

and Prof. Mohn, suddenly disappears. Not, however, without adding a sentence which I am sure he will in the end regret. He says he has "been forced by the personal attacks in which Mr. Croll has latterly thought fit thus to indulge to retort upon him." Why, the discovery of anything "personal" in Mr. Croll's writings would be as great a find as the true theory of oceanic circulation. I do not know any papers in our contemporary scientific literature more thoroughly undeserving of such a charge. Surely a man may call in question, nay, may even take a little quiet fun out of another man's opinions or crotchets without laying himself open to the stigma of being guilty of "personal attacks." Besides, it seems to me that Dr. Carpenter's charge is inappropriate. Mr. Croll, remarking "with some reluctance" that he was "compelled to refer" to Dr. Carpenter's continual quotation of eminent physicists who had adopted his views, while none had shared in the objections to them, merely assured Dr. Carpenter that such was not the case, and made reference to one person as an illustration, but without giving the person's name. The Doctor, as everybody knows, has been profuse in his use of this kind of argument. And now the moment it is used against himself, he denounces the introduction of "personal attacks!"

I purposely avoid entering into the merits of the question. What, in common with every sincere well-wisher of Science, I desire to see, is its thorough, honest and courteous discussion. Dr. Carpenter's high position gives a weight to what he says and does, which adds much to the regret with which his letter will be perused. That this protest may be received on its own merits and without reference to the pen which holds it, I withhold my name.

F. R. S.

Proportionality of Cause and Effect

MR. HAYWARD now affects the air of an injured man, and complains of being charged with "confusing issues" which he neither "raised nor accepted." He may be convicted out of his own mouth. The following passage occurs in his last letter but one (NATURE, vol. x. p. 25):—"It should be noted that my principal 'exemplification of *unconsciously-formed preconceptions*' was of Mr. Spencer's own choosing, namely, Newton's 'Second Law of Motion.'" In his last he says:—"The object of my remarks was simply to test the truth of a definite assertion of Mr. Spencer that 'the Second Law of Motion is an immediate *corollary* of the *preconception* of the exact quantitative relation between cause and effect.'"

Now let the words italicised be compared. In the first passage Mr. Spencer is said to hold that the Second Law of Motion is a preconception. In the second he is represented as maintaining that it is a *corollary* from a preconception. Is not this "confusing issues"?

Mr. Hayward has the choice of two alternatives. He may admit that one of these statements is a misrepresentation of Mr. Spencer's doctrine, as was alleged. Does he refuse to do this? Then he may transfix himself on the other horn of the dilemma, and boldly assert that in his view a preconception and a corollary from a preconception are one and the same thing. But until Mr. Hayward can arrive at some agreement with himself as to the terms in which he shall state Mr. Spencer's theory, the conclusion of impartial outsiders will probably be that he is not yet in a position to pronounce authoritatively on the merits of it.

"A Senior Wrangler" is good enough to say that my letter makes him feel "something like Alice behind the looking-glass." After this amenity, one may be pardoned for stating the position which *his* mental altitude leaves. A famous metaphysician once wrote an essay to prove that the narrow discipline of mathematics produces an incapacity for general reasoning. Sir W. Hamilton would have found his *à priori* arguments confirmed if he could have read the letter of "A Senior Wrangler."

The "Senior Wrangler" quotes a sentence of mine to the effect that "the experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved." He does not quote a previous passage in which it is said:—

"Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and

similar experiences that the idea of the relation of proportionality grows and becomes organic."

It might have been supposed that the doctrine so expressed was effectually guarded against misapprehension. Are not the preconceptions derived from the child's muscular experiences described as *numerically indefinite* (i.e. not expressible in *proportional numbers*)? Is it not said that out of them the idea of the relation of proportionality *grows*? In the very sentence quoted by the "Senior Wrangler," is it not said that the notion of proportionality is *implicated* in the child's consciousness, and that the physical axiom comes from this *latent* consciousness? And yet the "Senior Wrangler," looking down from his mathematical heights, and catechising me as he would a schoolboy, asks me whether I know "what proportionality means"?

But for the letter of a "Senior Wrangler," one would have believed that it was made clear to everyone that the notion of proportionality generated by these early experiences was vague and general, not exact. How else should I have said that from it "the axiom of the perfect quantitative equivalence of the relations between cause and effect is *evolved*?" After thrice reading "First Principles" does not the "Senior Wrangler" know that being evolved includes passing from indefiniteness to definiteness? How then can he pretend that it is meant that the child gets from his experiences the knowledge that a double effort produces in all cases just double the result? The argument obviously implied is that this is the *finished conception* finally arrived at by the adult, as holding in those cases where causes and effects are uncomplicated.

Having but limited space, and assuming that the requisite qualifications would be made by unbiased readers, I passed over all those details of the child's experiences which would have been required in a full exposition. Of course I was aware that in the bending of a stick the visible effect does not increase in the same ratio as the force applied; and hardly needed the "Senior Wrangler" to tell me that the resistance to a body moving through a fluid increases in a higher ratio than the velocity. It was taken for granted that he, and those who think with him, would see that out of all these experiences, in some of which the causes and effects are simple, and in others of which they are complex, there grows the consciousness that the proportionality is the more distinct the simpler the antecedents and consequents. This is part of the preconception which the physicist brings with him and acts upon. Perhaps it is within the "Senior Wrangler's" knowledge of physical exploration, that when the physicist finds a result not bearing that ratio to its assigned cause which the two were ascertained in other cases to have, he immediately assumes the presence of some perturbing cause or causes, which modify the ratio. There is, in fact, no physical determination made by any experimenter which does not assume, as an *à priori* necessity, that there cannot be a deviation from proportion without the presence of such additional cause.

Returning to the general issue, perhaps the "Senior Wrangler" will pay some respect to the judgment of one who was a Senior Wrangler too, and a great deal more—who was distinguished not only as a mathematician but as an astronomer, a physicist, and also as an inquirer into the methods of Science: I mean Sir John Herschel. In his "Discourse on the Study of Natural Philosophy," he says:—

"When we would lay down general rules for finding and facilitating our search, among a great mass of assembled facts, for their common cause, we must have regard to the characters of that relation which we intend by cause and effect."

Of these "characters" he sets down the third and fourth in the following terms:—

"Increase or diminution of the effect, with the increased or diminished intensity of the cause, in cases which admit of increase and diminution."

"Proportionality of the effect to its cause in all cases of *direct unimpeded* action."

Observe that, in Sir J. Herschel's view, these are "characters" of the relation of cause and effect to be accepted as "general rules for *guiding* and *facilitating* our search" among physical phenomena—truths that must be taken for granted *before* the search, not truths derived *from* the search. Clearly, the "proportionality of the effect to its cause in all cases of direct and unimpeded action" is here taken as *à priori*. Sir J. Herschel would, therefore, have asserted, with Mr. Spencer, that the Second Law of Motion is *à priori*; since this is one of the cases of the "proportionality of the effect to its cause."

And now let the "Senior Wrangler" do what Sir J. Herschel

has not done or thought of doing—*prove* the proportionality of cause and effect. Neither he, nor any other of Mr. Spencer's opponents, has made the smallest attempt to deal with this main issue. Mr. Spencer alleges that this cognition of proportionality is *a priori*: not in the old sense, but in the sense that it grows out of experiences that precede reasoning. His opponents, following Prof. Tait in the assertion that Physics is a purely experimental science, containing, therefore, no *a priori* truths, affirm that this cognition is *a posteriori*—a product of conscious induction. Let us hear what are the experiments. It is required to establish the truth that there is proportionality between causes and effects, *by a process which nowhere assumes* that if one unit of force produces a certain unit of effect, two units of such force will produce two units of such effect. Until the "Senior Wrangler" has done this he has left Mr. Spencer's position untouched.

Bayswater, May 20

JAMES COLLIER

The Great Ice-Age

IN reply to Mr. Belt's letter (p. 62), I did little more than express an adverse opinion to his theory, because to discuss it would have required an essay. I expressed this because I notice that unless something like a demurrer is entered against a new theory it is apt to be taken for granted in subsequent textbooks and papers written by those who have had no opportunities of obtaining a practical knowledge of the subject. For the above reason I must answer his strictures very briefly.

(1) I fail to see why the Scandinavian sea-beaches are irrelevant. (2) I have more than once read Mr. Tiddeman's paper, and without committing myself to all its conclusions, think I may quote it as assuming that the Lake district (as distinguished from North Lancashire) was the centre of a great ice-sheet; not that it was over-ridden by ice coming from somewhere further north. The same might be expected to be the case with the Welsh mountains; and Mr. W. Kingsley has brought forward good evidence of the existence of an ice-sheet there also. (3) Mr. Belt appears to forget that shells have been found not only at Moel Tryfaen, but also near Llyn Ffynnon-y-gwas, about two miles west of the peaks of Snowdon. Does Mr. Belt mean to say that Snowdon could not protect itself in the heart of its own domain better than this? If the Lake mountains had an ice-sheet, surely Snowdonia? Mr. Belt asks for evidence of the shore of the glacial sea. I reply that to me these and the Moel Tryfaen beds, not to mention others, appear to be far more probably littoral deposits than transported. For example, I think it in the highest degree improbable that the Vale Royal shells (Lyell, "Antiquity of Man," p. 317) could be brought to their present position (more than 1,100 feet above the sea) by any ice-sheet without the cold being enough to cover *all* the higher ground in Britain with ice, and so protect it. I did not deny a glacier might push a stone before it up-hill; my contention was that the enormous force which would be exerted on beds scooped out as described, and shoved some 1,500 feet up-hill for miles over broken ground, would crush the shells to a far more comminuted state than they are now in. With regard to Holderness, Mr. Croll's view of the shells there appears to me to be at present only a *theory* of which Mr. Searles V. Wood, jun., has effectually disposed (Geol. Mag. 1872). I grant there are some difficulties in the submergence theory; my position is that those in Mr. Belt's are very much greater.

A recent perusal of Mr. J. Geikie's suggestive book, the "Great Ice Age," has brought before my mind more strongly than ever a dilemma, which, as it appears to me, the modern school of Glacialists cannot escape.

He speaks of the till as a *grand moraine* or *moraine profonde* formed between the glacier and the rock, while he attributes the majority of rock-basins to the action of the glaciers. Now it appears to me that if the glaciers could pass over considerable deposits of this *moraine profonde* without sweeping it clean away, then their action as erosive agents must have been comparatively feeble; or, if they could scoop out great rock basins like the Alpine and (buried) Highland lakes, then they would have peeled off almost all the till from the land. As it appears to me, the analogy with a river, by means of which Mr. J. Geikie (p. 88) seeks to escape from a portion of this difficulty, does not hold. When a river begins to deposit sand and gravel largely, its work as an erosive agent at that place is almost over. Besides we cannot conceive a nearly solid mass, like a huge glacier, changing its motion so rapidly as a stream of water. Difficult as it undoubtedly is to explain some of the lake-basins, it appears to me that the great bulk of his evidence, with regard to till and

other deposits over which ice-streams have passed, shows how slight under ordinary circumstances is their erosive power; and this has been confirmed by every journey that I have made among the Alps. I may add also that from study of the same regions my faith in a *moraine profonde* is much shaken. I believe that, except possibly as a very local and exceptional phenomenon, it exists solely in the imagination of the eminent geologists of whom Mr. Geikie is a disciple.

T. G. BONNEY

St. John's College, Cambridge, May 26

Photographic Irradiation

IN the paper referred to by Prof. Forbes (NATURE, vol. x. p. 29) what is ordinarily called Photographic Irradiation was attempted to be explained by us, not as being caused by reflections from the back of the plate, but as being due to the sum of all the optical imperfections of the instrument with which the photograph is taken.

If Mr. Stillman (p. 63) will refer to our original paper, published in the *Monthly Notices* for June 1872, he will find that only the cloudy indefinite haze which surrounds the image of a luminous object, and which has frequently been called halation, was referred by us to reflection from the back of the plate.

When an over-exposed photograph is taken upon an opaque plate a marked fringe of irradiation still remains, and experiments were instituted by us which appeared to show that this is not to be accounted for by any circulation taking place within the thickness of the collodion or by the chromatic dispersion of the lenses; but when the oblique pencils from the edges of the lenses were stopped out the irradiation fringe was found to be greatly decreased. We were led to conclude that irradiation is to be accounted for by the fact that each luminous point in the object is not accurately represented by a luminous point in the image, but rather by a luminous patch of sensible area, the central and more intense portion of which prints itself first in the photograph, giving comparatively sharp picture prints when the exposure is short; but as the picture is still further exposed, the outer portions of the luminous patches imprint themselves, and by their overlapping cause the blurred appearance to which has been given the name of irradiation.

LINDSAY

A. COWPER RANYARD

Uncompensated Chronometers and Photographic Irradiation

WITH regard to the employment of uncompensated chronometers (NATURE, vol. x. p. 63), I have every reason to believe that the Russians alone have tested them. For some reason which is not easily discovered, the employment of a negatively compensated chronometer has not given any very remarkable results. The Russians have employed simply an uncompensated chronometer; and have obtained very remarkable results as mentioned in my article on the Transit of Venus to which Prof. Everett has alluded.

With regard to the prevention of photographic irradiation, of course various means have been employed for dry plates; but I believe that Lord Lindsay and Mr. Ranyard were the first to experiment on the matter exhaustively. I believe Mr. Stillman would be interested in reading their paper in the *Monthly Notices*. At the same time all honour is due to the photographers named by him for their experiments.

Birkenhead, June 1

GEORGE FORBES

The Seal Fishery

CAPT. DAVID GRAY, of the steamship *Eclipse*, has done good service to the cause of humanity in writing, and Mr. Buckland in publishing, the letter on the seal fishery which appears in *Land and Water* for May 9. The fearful cruelties perpetrated year after year, and the enormous waste of life entailed by the reckless manner in which the seal fishery is prosecuted, are well known, but no steps have hitherto been taken to regulate a trade which, if carried on within proper bounds, would continue to yield great profits, but if still pursued with such utter disregard to consequences must soon end in the extermination of the whole race. As an instance of the wastefulness of the mode of proceeding, Capt. Gray says that five ships attacking a pack of seals, in four days killed about 10,000 old seals; "add 20 per cent. for seals mortally wounded and lost, gives an aggregate of 12,000 old ones; add 12,000 young which died of starvation, gives 24,000; but this is not all. The men spread on the ice, so that the old ones that were left alive could not get on to suckle their young. The consequence was that the whole of the young